

Interactive comment on “Modelling Atmospheric Mineral Aerosol Chemistry to Predict Heterogeneous Photooxidation of SO₂” by Zechen Yu et al.

Anonymous Referee #1

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This paper presents an interesting contribution aiming at the development of new numerical model of dust (photo)chemistry. It describes in some detail the different key process that are implemented. Clearly, the use of surface photochemistry is a novel and valuable contribution. However, a few aspects of this modelling studies are raising a few important questions.

The partitioning between the gas and condensed phase are treated in a similar way, despite being fundamentally different in nature. For a solid surface, the adsorption and desorption processes do follow a different formalism, typically through a Langmuir-Hinshelwood formalism which takes into account a given number of adsorption sites. The consequences is that adsorption decreases with time or increasing concentration,

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while here it is simulated in a constant way with time... how can you justify such an assumption? Also products such as sulfate are probably staying on the surface, hereby also using adsorption sites i.e., poisoning the surface. How would your model change if you implement such time/concentration dependence?

Why having chosen to simulate deliquesced sea-salt and dust in the same code? What is the link between both objects/themes? Can you justify such a choice? Also the text mentions sea-salt and the partitioning process is described for aqueous sulfuric acid particles. Please harmonize the different part of your manuscript.

The characteristic time for adsorption is very different between aqueous and dust particles. Is this physically justified, bearing in mind that those processes are mostly related to gas phase and aerosols properties.

Too many rate constant are estimated without any justification. Please justify and explain your estimations.

The agreement with the chamber data has to be described in a more quantitative way. By looking at the figures, one may have the impression that the agreement is not as good as described in the text.

Without these changes, I do not think that this manuscript is suitable for publication in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-120, 2017.

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